SCIENCE

A WEEKLY NEWSPAPER OF ALL [illegible] AND SCIENCES.

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SCIENCE

NEW YORK, MAY 27, 1892.

A KEY TO THE MYSTERY OF THE MAYA CODICES.

I WISH to announce through *Science* to those interested in the subject, that I have fortunately discovered, at last, the key which will unlock the mystery of the Maya Codices and, probably, of the Central American inscriptions. The progress of decipherment will be slow, but, the clue having been obtained, it will ultimately be accomplished. I have already determined the signification of some dozens of characters and in several instances ascertained the general sense of a group forming a sentence.

This discovery settles at the same time several other points. First, it shows that the direction in which the Codices are to be read is as assumed by me in the "Study of the Manuscript Troano," pp. 136–141. Second, that the parts of the compound characters are to be read chiefly in the same way;

that is, from left to right and from the top downward. It shows, in the third place, that, although there are a number of conventional symbols, yet the great majority of the characters are truly phonetic, and the writing of a higher grade than has been hitherto supposed. Last, it shows that, after all, Landa's statements in regard to the mode of writing and the letters and characters are, to a large extent, correct. For example, his second *b* is correct if a central dot is inserted, giving five instead of four. His c is also correct, as are his *e*, *i*, and *ca*, his *k*, *ku*, *z*, *ha*, *ma*, and sign of aspiration. The *l* as given in his example of the mode of writing is correct. His first *x* (dz), if placed horizontally and slightly modified, is the symbol for *ch*'.

Landa's trouble as to the Maya mode of spelling, where he assumes that *le* is written thus, *ele*, arises from the fact that the beginning of the symbol for *l* is so nearly like that for *e*, that he has mistaken one for the other, thus considering the first part of the *l* as an *e*. This can be shown, as symbols for the same word, having the same meaning, are found at one point in the Codex Troano.

As one result of this discovery, I will introduce here an example, with illustration from page 32 of the Cortesian Codex. In the figure here shown the reader will observe a character in the hand of the human being represented as grasped in the mouth of the serpent and also one from which the serpent seems to rise. The latter is the symbol for *cab*, which in the Maya language signifies both earth and honey, here undoubtedly earth. The one in the hand of the human figure is a compound symbol for *yeb* or *yeeb*, signifying mist, dew, or humidity. We also observe in the eye of the human head a cross, which, like the serpent, is a rain or moisture symbol; thus agreeing with the view which has been advanced in regard to the signification of these symbols.

Without further reference at present to the discovery, I may say that I am preparing specimens of my interpretations and explanations, to be submitted to some of our leading archæologists and linguists.

In concluding, allow me to say that if I am correct in the above deductions, which have been reached after careful examination and tests, the Bureau of Ethnology, of which I have the honor to be a member, may claim to have rendered probable the solution of two important questions relating to the pre-Columbian times of our continent, to wit: Who were the mound builders? and, What is the significance of these curious Central American inscriptions and Maya writings?

CYRUS THOMAS.

Washington, May 17.

DR. D. H. STORER'S WORK ON THE FISHES.

SUCH of Dr. Storer's papers as have come to my notice, some of the minor articles possibly being overlooked, indicate that his activity as an ichthyologist extended over a period of about thirty years, beginning about 1836. His list of publications on the fishes is not a long one, and his standing amongst the workers of his own period, or of later periods, in this department of science, may be determined entirely from the latest, his greatest work, "The History of the Fishes of Massachusetts."

1. The earliest paper noted is entitled "An Examination of the 'Catalogue of the Marine and Fresh-Water Fishes of Massachusetts,' by J. V. C. Smith, M.D.," in Professor Hitchcock's "Report on the Geology, Mineralogy, etc., of Massachusetts." This appeared in Vol. I. of the *Boston Journal of Natural History*, pp. 347–365, pl. viii., occupying some eighteen pages, and bearing date of May, 1836.

2. In July, 1839, he published his "Remarks on the 'Natural History of the Fishes of Massachusetts,' by J. V. C. Smith, M.D.," in Vol. XXXVI. of *Silliman's American Journal of Science and Arts*, pp. 337–349, previously read before the Boston Society of Natural History at its meeting on March 20 of the same year.

3. His Reports on the Ichthyology and Herpetology of Massachusetts make an octavo of 253 pages and three plates. This was issued in connection with the report on the Birds, by Mr. Peabody. The Report on the Fishes was also pub-

lished in the *Boston Journal of Natural History*, Vol. II., pp. 289–558, where it differs very little from the separate. This report well represents the best American work done in ichthyology up to 1840.

4. In 1841 he published a short "Supplement to the Ichthyological Report," in the *Boston Journal of Natural History*, Vol. III., and in 1844, in the fourth volume of the same journal, his "Additional Descriptions of, and Observations on, the Fishes of Massachusetts."

5. The year 1846 saw the appearance of "A Synopsis of the Fishes of North America," an extensive work, mainly compilation, published in the Memoirs of the American Academy of Arts and Sciences, and reprinted separately, with different title-page, paging, and index, making a quarto volume of about 300 pages. In this work there are evidences that compiling was not so much to the author's liking as original work, in which he certainly attained a greater degree of success.

6. The "Catalogue of the Fishes of South Carolina" in Tuomey's Report on the Geology of South Carolina, of 1848, is a list of nominal species occupying several pages, for which dependence was placed on literature rather than on specimens.

7. In the fifth volume of the Memoirs of the American Academy of Arts and Sciences, 1853–55, Dr. Storer put forth the first, second, and third instalments of "A History of the Fishes of Massachusetts." The fourth part appeared in Vol. VI., 1858, the fifth in Vol. VIII., 1863, and the last in Vol. IX., 1867. The whole was published separately as a handsome quarto of 287 pages and 39 plates. This work contains descriptions and drawings taken from specimens of more than 130 species, together with a great mass of detail concerning habits, capture, economic value, and the like.

To show how the author regarded his own work we may quote the following, the opening paragraphs of the History:

"As one of the Commissioners on the Zoology of Massachusetts, in the year 1839, I prepared a Report on the Ichthyology of the State. From the brief time occupied in its preparation, it was necessarily imperfect, and, not being accompanied by figures, was comparatively useless, except to scientific men. Since the appearance of that communication, much information has been obtained respecting several of the most common and valuable fishes, and quite a number of new species have been ascertained to exist in our waters.

"Having carefully re-described all the species, I trust the following paper will present an accurate history of the fishes of our State. Considering this as the completion of my former report, I have kept in view the primary object of the commission,— to ascertain the value of our fauna in an economical point of view, rather than to prepare labored scientific descriptions."

The estimate placed by the author on his work in the report of 1839 may leave an imperfect idea of its real value. As he was engaged in revising and enlarging it, it was but natural for him to consider it not what it should be; yet for many years it was the standard work on our fishes, and was only supplanted in New England esteem by the revised, extended, and fully illustrated work completed in 1867.

It is through this last our author should be judged, all of the others being preparatory. Comparing the records included in its pages with the other records of the period, we shall have to rank it with the best. At the present, details are valued more highly, but to a considerable extent the details are supplied in the excellent drawings from nature, by the pencil of the artist, Sonrel, so long and so happily employed by Professor Agassiz. If we place this work on our own fishes by the side of those devoted to the fishes of other States; Mitchell's New York, 1818; Rafinesque's Ohio, 1819–20; Dekay's New York, 1842; Thompson's Vermont, 1842; Kirtland's Ohio, 1839–44; Baird 'sNew Jersey, 1855; Holbrook's South Carolina, 1860; or Holmes's Maine, 1862, we find but one or two that approach it and none that surpass. The excellence of the descriptions and illustrations is generally admitted. Taking up economic considerations, the work is readily seen to be in advance of any of the others. Being a forerunner of the fishery commissions, of either the general government or of the different States, Dr. Storer had to gather his statistical or other information directly from the markets or from the fishermen. One who has not engaged in similar work can hardly realize the magnitude of such an undertaking. In the evidence that accumulates there is apt to be so much that is more positive than accurate that at times it seems an almost hopeless endeavor to discover the truth. The Doctor, however, has acquitted himself admirably. He seems to have been especially fortunate in selecting the men on whom he depended most for assistance. Such names as those of Captain N. E. Atwood of Provincetown or Captain Nathaniel Blanchard of Lynn are often cited as authorities for statements of fact, and I have never yet been able to learn of a single instance in which their testimony has proved other than absolutely trustworthy.

The "History of the Fishes of Massachusetts" is a Classic in North American ichthyology that must serve as the basis for the future histories of New England's fishes. In the quarter of a century that has passed since its publication we have changed our ideals of names; and discoveries of new genera or species, or in the anatomy, have compelled changes in the arrangement. The nomenclature of the book has become somewhat antiquated, and the systematic arrangement is not entirely suited to the present time, yet we must say the same of all the contemporaneous ichthyological literature, and it will not be long before a similar characterization will be equally applicable to the works of to day. But it matters comparatively little to this book how much the names are changed, how radically the classification is modified, the things are described here, the illustrations are here, the facts are here, and these give the work a permanent value. It would be difficult to point out a work of greater accuracy in detail, or one that left less doubt in regard to the identity of the different forms to which attention is directed.

Dr. Storer was not led astray by desire for novelty; he used little of his energy in searching for generalizations; he appears rather to have given himself up to the careful preparation of a good record of what he could gather during years of collection and study. Most will admit that in this his judgment was good. For, though it sometimes happens that science is benefitted and fame is brought to an author by a revolutionary change in classification, or through a brilliant generalization or theory, the result most often is only an evanescent notoriety that soon dies away. It is through the patient elaboration of facts and success in recording them that one is most certain of contributing to the advancement of science. In this way Dr. Storer has made a contribution to ichthyology of lasting importance. In the amount of information given, its accuracy, and style of presentation, he has established his claim to present and future gratitude and has proved his right to rank amongst the foremost of American ichthyologists.

S. GARMAN.

MUS. COMP. ZOOL., CAMBRIDGE, MASS.

THE RELATION OF BUSINESS TO COLLEGE EDUCATION.

THE question of the benefit and advantage of a college education in relation to business was some time ago freely discussed in some of our leading magazines. Many of the articles were not without a grain of truth, yet in some of them the authors seemed to disregard certain essential questions.

It is not the purpose so much of higher education to fill the mind with knowledge as it is to discipline and develop the mind, and it is not so much the object of the college to make professional men as it is to prepare the student for a professional career.

An inquiry into the psychical constitution of normal beings will show some marked general characteristics. For example, some possess great executive powers associated with marked powers of application and execution; others possess great reflective powers associated with slight powers of execution and little or no executive ability; and others again may possess all these powers equally strong, or they may be variously combined in individuals as to degree and quality. The intention is not to enumerate any more characteristics than such general ones as come into use in business. All these powers admit only of a certain degree of development.

It is conceded that every normal being is endowed with certain natural abilities to acquire knowledge. The degree of the development of these abilities and the direction in which they run are often difficult to determine; and here it is where the largest number and most serious mistakes are made. Every individual, no matter what his abilities are, must receive a certain amount of training and education, and these may come to him in one form or another, either practical, theoretical, or both. The acquisition of knowledge through personal experience alone will prove both good and bad, and it is through a theoretical training and education that the bad may be avoided. In discriminating between all the degrees of natural abilities supplemented with practical training and natural abilities supplemented with book learning, an important element will be found which can only be acquired through systematic book training and proper schooling, and that is discipline. Discipline gives discriminating powers and quickness to the perception, lends accuracy to the conception, aids the reason to draw proper conclusions from a series of facts, and thereby sharpens the judgment, develops the memory, controls the will, and subjects the emotions. In connection with this, the attention may be called to the fact that a methodical mind is not always a well disciplined mind.

Again, there is a distinguishing element common only to natural ability, and which does not depend upon knowledge alone or any higher psychical organization called knack. This is generally noticeable in powers of execution. For example, if we observe a number of mechanics working at the same job, it will appear that a few of them show exceptional facility in the execution of their work, while others with all their training and practice cannot attain this facility. The degree of difference in the work may not be great, yet it is noticeable; and the man who is the happy possessor of this particular gift is the man most sought for.

A comparison of various individuals within the different classes in which men may be classified from the standpoint of vocation will show similar results. As an illustration we need only to take two orators. Suppose them as nearly as possible to be equally equipped mentally, morally, and physically, yet when addressing an assembly there will be seen a marked difference in the effect produced upon the audience. To the one they will listen with indifference; to the other they will appear as if they drank in every word that fell from his lips. So again, if two orators unequal in education and training address an audience, it is not infrequent that the one possessed of the lesser education and training will hold his audience spell-bound, while the other will leave his audience cold and unaffected. The question will now arise, To what can this difference be attributed? The solution undoubtedly lies in the peculiar, fascinating influence exercised over the listeners through the method, the style. It is this which inspires truthfulness, conviction, and confidence, and may be considered a quality of executive ability. In every vocation of life we may trace this quality as essential to success. The author, the poet, the lawyer, the actor, the politician, the merchant — all of whatever class will profit by possessing this quality. True, this quality may be developed to a limited extent, yet the possessor of it by nature need have hardly any schooling or training, and he will succeed.

The questions, which will now present themselves for consideration, are, Why is it that so many men of very inferior mental capacity and in some instances of marked natural ability, though uneducated, are so successful in accumulating large fortunes, and why is it that so few college-bred men are successful in the commercial world and become possessors of large fortunes?

It is not infrequent where men equally equipped mentally, either educated or uneducated, start out in the world both having the same habits of thrift and economy, of industry and energy, of perseverance and endurance, and both having equally good opportunities for making money, that one of them succeeds in accumulating a large fortune, while the other gets along but moderately. The statement is quite generally admitted that a person who is economical, prompt, reliable, honest, and accommodates himself to the circumstances, and does not meet with any misfortune, may acquire sufficient means to live fairly well, but to acquire a large fortune something more is requisite.

To what this difference may be ascribed is the question. Surely, it cannot be maintained that one has more brains than the other or that he possesses better advantage by whatever means, for the assumption is that they are equal in these respects. Then the only factor to which this can be attributed is unquestionably the style or peculiar influence they exercise over others, and by which they inspire confidence and enlarge their circle of patrons. As an argument in favor of this view a reference to cases where men are totally unworthy of confidence needs only to be made. How often does it happen that men morally perverted are capable of inspiring confidence in people, and this not only in the unwary and ignorant but in men of brains and education. How often do men of inferior intellect exercise much influence among the educated and ignorant. To attribute this to any other power than the peculiar fascinating influence that many men have over others is absurd. The average business man, however great his success may be, and who has received no college training, is narrow, emotional, exacting, and will often resort to means in accomplishing his purposes which a college educated man would hesitate to do, and most college-bred men of this stamp will possess these traits inherently.

That the school of experience quickens self-reliance, that

it gives positiveness to one's opinions and conduct, that one more readily forms his final conclusions from first impressions, and that a well disciplined mind might avert many sad experiences, which an undisciplined mind is obliged to go through, is undisputed. That the college graduate has many edges to round off when he enters upon the struggle for existence is manifest. During his entire college course he has only heard of the highest standards of the intellect and of morality. Although he has been taught to deal with things as they are, yet a large portion of his instruction has been devoted to things as they should be; and therefore when he starts in life he must adjust himself to life as it is. Whether this is a fault in that the training is not held within the limits of the practical may be an open question. But on the other hand, that a college education has the tendency to make one more humane, to broaden one's views of life, to make one more liberal, to quicken one's perception, to lend accuracy to the judgment, and insure more logical thinking, cannot be denied.

Franklin A. Becher.

THE SYSTEM OF ALGOL.[1]

The steady advance of exploratory research in the system of Algol promises to furnish one of the most curious and instructive episodes in the history of science. Vague hypothesis, determinate theory, and triumphant verification have already played their logically sequent parts in the discovery of the eclipsing satellite. Goodricke's conjecture, however, had to wait nearly a century for Pickering's formulization, while this was ratified within a decade by Vogel's disclosure of the anticipated tell-tale spectroscopic effects.

Progress has, indeed, of late notably quickened its pace; and we may therefore hope for a prompt and effective application of the Ithuriel-spear of adapted observation to the latest creation of speculative intelligence in the lately organized department of "dark stars." Since Argelander's time it has been tolerably evident that Algol had other attendants besides the agent in producing its periodical eclipses. For their recurrence was shown by him to be subject to minute irregularities in point of time, and these irregularities are of such a nature as to demand for their explanation the presence of at least one disturbing mass. A highly complex piece of mechanism could plainly be seen to be at work; yet the penetration of its intricacies presented a task so formidable that astronomers of, at any rate, the present generation might well have despaired of its accomplishment. It has, nevertheless, been undertaken by Dr. Chandler, and his labors have been rewarded with an encouraging measure of success.[2]

They have been necessarily of a more or less tentative character, and their result must be looked upon as merely provisional; but there is much reason to suppose that it at least approximates to the truth. It is, moreover, perfectly plain and straightforward; there is nothing of the *obscurum per obscurius* about it; the consequences it involves are definite, and admit of definite verification.

The new and enticing hypothesis now presented for the consideration of astronomers is mainly founded upon certain well-ascertained inequalities in Algol's period of variation. These were shown by Dr. Chandler's discussion some little time since[3] to be slowly compensatory. They are oscillatory, not progressive. Consistently in advance of their due time down to about the year 1804, the obscurations of the star then began to fall behind it, and the delay had accumulated in 1843 to 165 minutes. A gradual process of restoration thereupon set in, and the normal epoch was reached near the beginning of 1873. It was quickly, however, transcended, for acceleration is still going forward, and is likely to continue operative during some years to come.

These irregularities are evidently comprised in a cycle considerably exceeding one hundred years, and for that very reason it is difficult to account for them on gravitational principles; since a third body, exterior to the close pair, should, in order to produce any marked purturbational effects, revolve much nearer to them than would be consistent with so long a period. Another mode of explanation is, accordingly, resorted to by Dr. Chandler. The varying intervals needed for the transmission of light from different parts of a large orbit described by Algol and its dark satellite round a remote primary, are, in his view, the fundamental cause of the alternate anticipations and retardations in the occurrence of Algol's eclipses. They are, in fact, apparently shifted backwards and forwards in time, just in the same way as are the eclipses of Jupiter's satellites through the orbital movement of the earth. Algol may, then, be regarded as the solitary luminous number of a multiple combination of opaque masses. The common centre of gravity, round which the pair hitherto known revolves in a period of about 131 years, lies by the present hypothesis at a distance from it just equal to that of Uranus from the sun. The path thus traced out is, we are further informed, sensibly circular, and its plane is inclined 20° to our line of vision. Obviously, however, during the whole time occupied in travelling over its remoter half, the light-minima of the star must be recorded somewhat later than if we saw them in the precise order of their actual occurrence; and this remoter half was swept over between the years 1804 and 1869, when the observed phases were always in arrear of calculation. Now, on the other hand, that the star is on the hither side of its orbit, the epochs of its eclipses are apparently anticipated, and will not coincide with their true times until the passage of the "ascending node," about 1934. The dimensions of Algol's orbit, with its inclination, of course prescribe the amplitude of the oscillations by which its periodicity appears to be disturbed; and this "light-equation," as we may call it, proves to be 149 minutes. This should be the maximum extent, whether of acceleration or of retardation; but in point of fact, as we have seen, delay mounted up in 1843 to 165 minutes. Hence the theory cannot be said to represent the observations as satisfactorily as could be desired. The deviations, indeed, are large enough to suggest to Dr. Chandler further complications, the unravelment of which may challenge the utmost skill and patience of investigators. Meantime, a touchstone of the general truth of his hypothesis will soon be at hand; for it involves a cessation within the next ten or twelve years, and a subsequent reversal of the shortening process at present affecting the star's period of luminous change; and the fulfilment of this prediction will serve as a hall-mark of its genuine quality. An additional test may be derived from the spectrographic evidence. The velocity of Algol in the large orbit attributed to it is 2.7 miles per second; but of this, less than one-half, or about one mile per second, is at present directed towards the earth. It constitutes, however, a goodly proportion of the 2.3 miles of continuous approach determined from the Potsdam plates; but which should in the course of a score of years, if the new theory be true, completely disappear, neutralized by the altered direction of the star's orbital motion. It remains,

[1] From Knowledge for May.

[2] Astronomical Journal, Nos. 255, 256.

[3] Ibid., vol. vii., pp. 153-183.

indeed, to be seen whether the whole of its supposed translatory speed may not really be of a circulatory character.

Dr. Chandler's theory does not rest wholly on the cyclical inequalities of Algol's light-changes. He alleges also in its support periodical disturbances of proper motion, brought to view by a careful discussion of all the observations of the star, from 1753 to the present time, and indicating, in his opinion, a combination of elliptical travelling with a progressive advance. But the average proper motion of Algol is so very small—less than 2″ of arc a century—that variations or irregularities in it can at present be regarded only as an interesting possibility. They would give, if confirmed, 2.7″ for the longest diameter of the ellipse into which the wide orbit traced out by Algol round its unseen primary is projected upon the sky. And since this little span represents an actual expanse of 38 earth-to-sun distances, or "astronomical units," it implies a parallax for the star of 0.07″, corresponding to a distance of nearly 47 light-years—a statement that is in many ways worth thinking about. Although claiming only qualified credence, it nevertheless conveys the upshot of assuredly the most promising attempt yet made to determine, by indirect means, the parallax of a star. In itself, too, it seems probable enough. Assuming its accuracy, we gain the information that Algol emits 63 times as much light as the sun, which, in its place, would show with little more than the brightness of a seventh-magnitude star. The famous variable, moreover, according to Dr. Vogel, is just one million miles in diameter, so that it presents only once and a third the solar radiating surface; yet it is, as a light-giver, 63 times more effective. The remarkable conclusion follows, that Algol is intrinsically 47 times more brilliant than the sun. The emissions from its photosphere are, per unit of area, 47 times more powerful. And should its parallax eventually—as seems not unlikely—prove to be smaller than 0.7″, this disparity will be still further enhanced.

By means, accordingly, of investigations of this nature, more fully and securely carried out, the question as to comparative stellar brilliancy may finally obtain a sufficiently satisfactory answer. It is a very important one. The process by which photospheric light is manufactured is still largely enigmatical, but the ideas commonly entertained about it are not easily compatible with the existence of considerable differences in the shining faculty of photospheric shells presumably identical in point of chemical composition. Reliable evidence of such differences has not hitherto been available. That light-power in stars bore no fixed proportion to mass was patent in numberless examples; but the density, consequently the dimensions of the emitting bodies remaining unknown, it could not be determined whether distension of substance, or innate strength of incandescence, was more concerned in producing a great sum-total of light relative to quantity of matter. The indications, however, now derived from Algol are overwhelmingly in favor of the latter alternative.

The primary member of its system, even if illuminated solely by the borrowed rays of its brilliant neighbor, may not, Dr. Chandler thinks, be out of reach of telescopic discovery. But his hopes, in this case, appear somewhat chimerical. It is not difficult to show that, under the circumstances supposed, a body of planitary constitution could not possibly be disclosed by any optical means at present available. Its position-angle relative to Algol is just now, we are told, 32°, while its distance from the same star is in the inverse ratio of its mass. This is considered by our author to be indeterminate; but it is not so, unless we reject Dr. Vogel's value for the combined mass of the close pair forming the variable. Assuming its approximate correctness, and that Algol and its immediate attendant accordingly contain two-thirds the solar quantity of matter, and admitting, further, that they revolve together, at a distance of nineteen astronomical units, in a period of 131 years, round their common centre of gravity with another body, it follows that the mass of that body is about equal to that of the sun, and that it circulates at twelve units of distance from the gravitational centre of the system. It should be found, this being so, if found at all, at an apparent interval of rather less than 2″ from Algol. The real gap of space separating them—the radius, that is to say, of Algol's relative orbit—would be measured by thirty-one radii of the earth's orbit; and the effectiveness for visual purposes of a still problematical body, shining by reflected light alone, can hence be estimated. If of the same density with Algol, it presents a disc of five-fold area, which, endowed with Jupiter's high reflective power, or an albedo of 0.62, would possess a total lustre $\frac{1}{7,680,000}$ that of the original source of its radiance. This is equivalent to saying that it should be fainter by sixteen stellar magnitudes. Yet the suppositions introduced above are perhaps unduly favorable to conspicuousness. Evidently, however, an eighteenth-magnitude star, in the close vicinity of one of the second, is far below discernment with any telescopic or photographic powers likely to be in use for a considerable time, if ever; so that visual confirmation of Dr. Chandler's theory can only be looked for if the unknown mass it has brought ideally into existence be in some degree self-luminous.

That theory, as he remarks, "has a much wider cosmological meaning than the mere explanation of the phenomena" of a single star. Most "eclipse-variables" exhibit irregularities of the same type with those of Algol, and which will doubtless prove amenable to a similar explanation. Moreover, an incalculable number of stars which, from our point of view, escape eclipse, unquestionably belong to systems organized on the same general plan. One such, indeed, is already known in α Virginis, a first-fruit of discovery in this particular branch; and Procyon, perhaps, is one of many others essentially resembling it, although inaccessible to spectrographic research, because revolving in planes nearly perpendicular to the line of sight. Thus the intimate association of dark and bright bodies of the same order of mass would appear to be no exception in the universal order. And this scarcely allows us any longer to regard a sun-like condition as representing simply and solely a stage in the condensation of a primitively nebulous mass. Some further conditions are plainly needed to produce the brilliant and concentrated evolution of light characteristic of "suns."

Dr. Chandler concludes his valuable paper with an appeal for micrometrical measures of Algol stars, adapted to detect and determine possible systematic disturbances of their proper motions. Measures of the kind might, in his opinion, lead to highly significant results, which would probably, in the case of Y. Cygni, be reached with particular promptitude. "If the research gave favorable results in this instance," our author continues, "it could then be extended to λ Tauri, which appears to be also a promising candidate." It is to be hoped that the suggestion will not remain unheeded. Owners of heliometers could hardly turn them to better account than by applying this simple criterion to an hypothesis which opens yet one more road through the daily widening field of sidereal discovery.

MISS A. M. CLERKE.

SCIENCE:

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THE CHANGE AT CORNELL.

The resignation of Dr. Charles Kendall Adams, president of Cornell University, dated May 5, the acceptance of that resignation by the Trustees at a special meeting called for that purpose May 18, and the immediate election of Dr. J. G. Schurman, Dean of the Sage School of Philosophy at Cornell, to fill the vacancy thus arising at the end of the current college year, are events of supreme importance to that institution, and, we are inclined to think, to the cause of education, and especially of scientific and technical education, generally. President Adams states, as the reason for his withdrawal, that differences of opinion in matters of essential importance in the management of the institution divide the authorities, and finding himself out of harmony with the majority of the managing body—the local Executive Committee, presumably—he feels it his duty to turn the office over to the Trustees.

What are the details of these differences is not stated by him, nor are they publicly known, and conjecture in so important a matter is only harmful; it is sufficient that they must be radical, to bring about such a change. Meantime Dr. Adams has been at the head of that great University seven years, and has seen the most extraordinary development in the course of its always remarkable and striking history. It is to be sincerely hoped that the new administration will be equally fortunate with that just closing. The student-body has increased in these last seven years to between two and three times the number present the year before the accession of President Adams. New departments have been created, new schools formed, and the whole system of organization greatly changed, usually in the direction of advancement. The Trustees, in accepting the resignation, assert that the retiring officer has exhibited wisdom, knowledge, and admirable discretion in his choice of professors, as well as in his general management of affairs, and tender him a year's salary as a testimonial—a very practical one—of their indebtedness to him, and also request that he sit for his portrait as an addition to the gallery representing the already long list of benefactors of the University. He has certainly a most satisfactory period to review in his final report.

From correspondence in our columns, during the first year of President Adams's administration, and from other sources, we might have had reason to anticipate anything but satisfactory encouragement of Cornell's leading objects. Cornell, it will be remembered, is a "land-grant college" for technical education and scientific work. But the results do not at all encourage that idea. The scientific departments have continued strong, and have grown fully as rapidly as the classical and the literary; in fact, in some directions their growth has been even more extraordinary than that of the latter. The courses in arts and in civil engineering have substantially the same number of students; in architecture the growth has been continuous and rapid; and Sibley College, the departments of which are mainly devoted to instruction in the main lines prescribed by the foundation and by the founders of the University as a school of mechanical engineering and the mechanic arts, has gained, according to the figures of its monthly journal, one thousand per cent. In physics, and especially in the physics of engineering and of electric light and power distribution, and in chemistry, especially in chemistry applied in agriculture, the work performed in research as well as in instruction has attracted general attention, and has done much to place the University among the leading institutions of its class. Its leading objects have been promoted as remarkably as those presumably much nearer the heart of the outgoing president. There is, however, considerable discrimination against the technical courses at Cornell; the charges for tuition being about fifty per cent higher than in the general courses, and their progress has been the more remarkable for this fact. Whatever the reason for his surrender of his charge, there is no question that President Adams has the privilege of looking back upon a most enviable period of great opportunity well-availed of.

Dr. Schurman, the new president, is a very young man to carry such responsibilities— but 38 years of age; but he is reported to have the strength, the energy, and the good-temper of healthful youth, to be capable and even a genius in administration; to be in full sympathy with the work which his acceptance of the position pledges him to carry out in accordance with the terms of the Law of Congress, the Charter of the University, and the explicitly stated wishes of its greatest benefactors; and to be liberal enough to give satisfaction to the officers charged with the conduct of the principal departments of the University. He has the confidence of the Trustees, as was evident from their unanimous agreement in his selection; and it may probably be safely anticipated that Cornell will, under his administration, continue to grow with a rapidity only limited by the magnitude and permanence of her income. Like all great institutions of her class, she always has larger demands than her purse can meet, and her opportunities grow faster than her income. New York State is an exception to the rule in this matter. Nearly all the States, especially those west of New England, make permanent and liberal provision for their land-grant colleges; but New York has never, we understand, done anything for her now flourishing but yet needy State University. One of the opportunities of the Schurman administration may perhaps be the establishment of closer relations with the State, for which his charge is doing so much, and from which it is receiving so little.

HOW TO PROTECT INVENTIONS IN FOREIGN COUNTRIES WITHOUT EFFECT UPON THE TERM OF UNITED STATES PATENT.

ACCORDING to late articles in daily, and even certain electrical and other scientific papers, and according to current remarks of inventors, a prevalent idea seems to exist to the effect that it is detrimental to the inventor's interest to obtain foreign patents, because the duration of his domestic patent will be shortened. This inaccurate and misleading understanding of the law is employed as an argument in favor of the revision of the patent statute relating to the maximum and minimum terms of patents. The object of this article is not to argue concerning such a revision, but it may be stated that the writer is greatly in favor of revision, but would emphasize that misrepresentations of the present law will rather hinder than further revision. To say, in general, that a United States patent expires with the term of the patentee's foreign patent, is misleading, because it is true only in some instances. Such statements are made, and the conclusion arrived at by the ordinary inventor is such as to apparently convince him that foreign patents are very dangerous and had better be left alone.

It is probably impossible to compose one sentence which will convey the exact relations of the terms of patents, because of the multitude of variations or differences among the patent laws of the numerous countries in which inventions may be protected with profit. Classifications somewhat as follows will, it is thought, make the matter so plain that the ordinary inventor may easily use the same for reference, although he could not, probably, remember them very accurately from one reading. All important phases are set forth, because it is not enough to know simply the effect of foreign patents upon the term of the United States patent, but upon one another's terms. In order to be brief, the language is intended to be such as to convey concise and practical information to inventors, independently of historical developments, irrelevant conjectures as to future decisions, and other matters valuable only to the mere student and patent attorney.

In each list which follows, the countries are named alphabetically to facilitate reference.

The term 17 years of a United States patent is *not* shortened: —

By any simultaneous, or subsequent foreign patent;

Nor by a prior patent in Belgium, United States of Columbia, Liberia, or Spain, provided the foreign patent is not over three years old;

Nor by a prior caveat in Argentine Republic, British Guiana, British Honduras, Canada, Great Britain, Sandwich Islands, Leeward Islands, Queensland, Russia, South Australia, Switzerland, Tasmania, Trinidad, Victoria;

Nor by an application filed, within seven months of the United States application, in Belgium, France, Great Britain, Guatemala, Italy, Netherlands, Norway, Portugal, Servia, Spain, Sweden, Switzerland, and Tunis;

Nor by an application filed, within six months of the United States application, in Brazil or San Domingo.

The maximum amount of reduction of the term of a United States patent by a *prior* foreign patent is equal to the difference of the term of 17 years and that remaining term which the foreign patent has to run. The maximum terms of patents in foreign countries (leaving out those of longer term than 17 years) are: Argentine Republic, 15 years; Austria, 15 years; Barbadoes, 14 years; Brazil, 15 years; British Guiana, 14 years; British Honduras, 14 years; Canada, 15 years; Cape of Good Hope, 14 years; Ceylon, 14 years; Chili, 10 years; United States of Columbia, 10 years; Denmark, 5 years; Ecuador, 15 years; Fiji Islands, 14 years; Finland, 12 years; France, 15 years; Germany, 15 years; Great Britain, 14 years; Guatemala, 15 years; Sandwich Islands, 10 years; Hong Kong, 14 years; India, 14 years; Italy, 15 years; Jamaica, 14 years; Leeward Islands, 14 years; Luxumburg, 15 years; Mauritius, 14 years; Mexico, 10 years; Natal, 14 years; Newfoundland, 14 years; New South Wales, 14 years; New Zealand, 14 years; Norway, 15 years; Paraguay, 10 years; Peru, 10 years; Portugal, 15 years; Queensland, 14 years; Russia, 10 years; South Australia, 14 years; St. Helena, 14 years; Straits Settlements, 14 years; Sweden, 15 years; Switzerland, 15 years; Tasmania, 14 years; Trinidad, 14 years; Turkey, 14 years; Uruguay, 9 years; Venezuela, 15 years; Victoria, 14 years; West Australia, 14 years.

A valid patent is not obtainable in Ecuador, France, Germany, Leeward Islands, Luxumburg, Peru, Sandwich Islands, South Australia, Switzerland, Turkey, or Venezuela, after a prior patent has been issued in the United States, except in the case of France and Switzerland, under the condition that the application is filed within seven months after the United States application was filed.

A valid patent is obtainable, in Canada, if applied for within 1 year after issue of United States patent; in Italy, within 15 years; in Spain, within 2 years; in Argentine Republic, within 10 years; in Victoria, within 1 year; and in Western Australia, within 17 years.

In countries not named in the two paragraphs preceeding, valid patents are obtainable, as a general rule, if the inventions are not well known, or in use, within the territory of those countries.

From the foregoing facts, it is evident that, by planning the times of application, valid and useful protection may be obtained throughout the world without in any way shortening the full term in any country.

The point of law to be considered for revision, is the provision of a right of the American citizen to obtain a seventeen years' patent whether he has previously patented it abroad or not.

EDWARD P. THOMPSON.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

The General Circulation of the Atmosphere.

THE question concerning the origin of the winds, or the general circulation of the atmosphere, has been a perplexing one for many years, and is not yet completely and fully settled. There are so many factors entering into the question, that its solution is difficult to comprehend off-hand. Instead of a broad, flat plane, upon which idea some conclusions seem to be based, we have a globe, and the atmosphere is a complete envelope thereof, having almost the same spheroidal shape as the earth, upon which it rests. This envelope is made to adhere to the surface of the earth by means of gravitation, but not so rigidly that it may not be set in motion by the application of heat. The earth revolves on its axis daily, and the air revolves with it, although it does not always travel at the same rate.

A body of air at rest for some time, or moving only with a very slow motion, will soon acquire the direction of motion of those parts of the earth with which it comes in contact. When such a

body of air moves to the north or south near the surface, it soon acquires an eastern or western component of motion; and if the same body of air returns as the upper strata, the eastern or western component of motion is reversed.

If the temperature of the entire body of atmosphere from pole to pole and from top to bottom, were the same, it is believed there would be no motion whatever of the air. Heat, therefore, or difference in the temperature, is the prime factor in the generation of air currents. It is maintained by some writers, however, that the rotation of the earth upon its axis, from the west to the east, would propagate a current of air, in the opposite direction, at or near the equator. But this has not been demonstrated to the satisfaction of all. So also it is believed that as aqueous vapor gives buoyancy to the air, this might produce some gentle breezes. As the sun is the source of all heat, we must look to that luminary as the producer of all winds and air currents. If the earth's surface were all water, or all land with a homogeneous topography, the air currents of the earth would remain almost constant and uniform. This continuity and uniformity are broken up and interfered with by the various divisions of continents, oceans, mountains, sandy plains, etc., and also by the variations of the temperature at the same locality at different periods in the year.

As the earth revolves upon its axis, one-half of its surface is always in the sunlight and the other half at the same time in darkness. The temperature of the former therefore is always higher than that of the latter; and the atmosphere tends to flow from the one to the other at the surface, and in the upper strata to flow in the opposite direction. This is in obedience to the known laws of dynamics: 1. That heated air will rise, and the vacuum which this tends to produce will be filled up by cold air flowing in at the bottom. 2. As gravity tends to keep the air in equilibrium, when a current of air is observed to be moving in any direction, either horizontally or perpendicularly, a like current will somewhere be found flowing in the opposite direction, to restore the equilibrium.

Let us liken the two hemispheres of air (one in the light and the other in the dark) to two great thin metallic cups or bowls, each enveloping one-half of the earth, their edges touching each other and coinciding with a great circle or meridian of longitude. Let them be pivoted at the poles, so that they may slide around the earth, one following the other. The one in the sunlight we will paint white, and the other black; and they follow each other as the earth revolves, the white being always turned towards the sun. Again, as the sun always shines perpendicularly to the earth's surface between the tropics, we find here a broad belt abnormally heated; and we will represent this heat on the white bowl by a broad band of bright red, and on the black bowl by a similar band of dark brown. Further, in examining the air along the edges of the two bowls, we find but little difference in the temperature of the two, the air growing colder as we recede from the edge towards the centre of the black, and warmer in the direction of the centre of the white bowl. We will therefore shade these edges, at first both nearly alike, but gliding into a dark gray, and then black towards one, and a light gray and then white towards the centre of the other. Still further, as the sun is annually perpendicular at each of the tropics, so the two poles will alternately be in light and shade — heat and cold. We must therefore give some slight shading to these parts of the bowls, to represent the different amounts of sun energy employed to heat the earth at these points.

We have now before us, therefore, most of the factors that enter into the production of air currents, or the circulation of the atmosphere. If the air could be seen with the naked eye, as we have here colored and shaded these two enveloping bowls, and if we could stand upon the moon or some inter-stellar planet and look upon the earth, what a grand and magnificent kaleidoscopic panorama would be presented to our view!

The theory of the general circulation of the atmosphere most generally accepted is shown in the following extract, taken from Buffon's "Natural History," which he quotes approvingly from Maclaren: "The unequal distribution of heat over the surface of the land and water necessarily disturbs the equilibrium of the atmosphere, and produces currents of air, or winds. These currents, however various, have been supposed to result from two general movements, pervading the whole mass of the atmosphere. The heavy and cold air of the temperate regions, having a tendency to displace the warm and rarified air of the torrid zone, generates a current in each atmosphere" (hemisphere?) "towards the equator. To replace the air abstracted from the higher latitudes, an upper and counter current flows back from the equator to the pole; and thus the atmosphere, while it performs a constant revolution, tempers the extremes of climate, by transporting the cold of the frigid zone to the equator, and carrying back the heat of the equator to the frigid zone." A writer on the article "Winds" in "Chambers's Encyclopædia" says: "When the part of the earth's surface which is heated is a whole zone, as in the case of the tropics, a surface wind will set in towards the heated tropical zone from both sides, and uniting will ascend, and, there separating, will flow as upper currents in opposite directions. Hence a surface current will flow from the higher latitudes towards the equator, and an upper current towards the poles." Professor William Ferrel, author of a "Popular Treatise on the Winds," published in 1889, practically adopts the above views. This is an elaborate work, containing about 500 pages, and is considered one of the best authorities on the subjects treated. In Chapter III., from page 89 to 162, he gives a detailed and graphic account of the "general circulation of the atmosphere." He frequently refers to the upper strata flowing "from the equator to the poles," and the surface currents flowing "from the poles to the equator," etc. On page 154 he gives a "graphic summary," as follows: "In the preceding part of this chapter it has been shown that if all parts of the atmosphere had the same temperature there would be a complete calm over all parts of the earth's surface. But that in consequence of the difference of temperature between the equatorial and polar regions of the globe, and the consequent temperature gradient, there arise pressure gradients and forces, which give rise to and maintain a vertical circulation of the atmosphere, with a motion of the air of the upper strata of the atmosphere from the equator towards the poles, and a counter current in the lower part from the poles toward the equator, as represented by the arrows in the following figure, and that this of course requires a gradual settling down of the air from the higher to the lower strata in the middle and higher latitudes, and the reverse in the lower latitudes. It has also been shown that, in case the earth had no rotation on its axis, this would be exclusively a vertical circulation in the planes of the meridians, without any east or west components of motion in any part; but that, in consequence of the deflecting forces arising from the earth's rotation, the atmosphere at the earth's surface has also an east component of motion in the middle and higher latitudes, and the reverse in the lower latitudes; and that the velocities of the east components increase with increase of elevation, so that, at great altitudes, they become very much greater than those at the earth's surface; while those of the west components decrease with increase of altitude up to a certain altitude, where they vanish and change signs and become east velocities, now increasing with increase of altitude to the top of the atmosphere."

Now the foregoing theory seems to me not to be sustained by the facts. It may seem presumptuous in a layman to question the conclusions of such great and confessed authorities; but if I am to follow what seems to me to be the truth, I must dissent. I have a profound admiration for the untiring labors and great researches of Professor Ferrel; he has placed the world under many obligations for his valuable suggestions; yet I fear he has fallen into the same error which has characterized the reasoning of all his predecessors. This is not strange or unexpected; for we all know that sometimes grave errors will for generations run through the writings of the most astute and learned men, undetected. Witness, for example, Lord Bacon's "Wisdom of the Ancients," where his "explanations" need explaining, and are more abstruse and muddled than the mythology of the Greeks and Romans. I do not know that what I may here offer as the true theory of the circulation of the atmosphere is new to the scientific world, as I have not kept full pace with all the new discoveries. I know, however, that it is not recognized by Professor

Ferrel, even by a reference; and if it had been proposed, before the publication of his book in 1889, I have full confidence that he would have known it, and either endorsed it or attempted to refute it. But no lover of truth should blindly follow any leader, however great; if his reasoning does not convince his understanding, he should mark out a course for himself, if that be to him the truth.

Any theory of the physical universe, or of any of its parts, which will account for the greatest number of known facts involved, will be most satisfactory and acceptable; and such theory will be held and entertained until another is discovered which will account for more, or all, of the facts.

Now any theory of the general circulation of the atmosphere may be accepted tentatively, as a true theory, which will fully explain and account for the following facts:—

1. The trade-winds of the tropics.
2. The belt of calms at the equator.
3. The ascent of the trades to the upper strata at the equator.
4. Their flow as upper strata toward the temperate latitudes.
5. The belt of calms near the 30th parallel of north latitude.
6. The high barometric pressure at this calm belt.
7. The prevailing south-west winds in the north temperate zone.
8. The source of supply of the trades.
9. The source of supply of the constant (or prevailing) south-west winds in the north temperate latitudes.
10. The limit of range of the return trades of the tropics and the same limit of the upper strata in the north temperate latitudes.
11. The calm belt about the 60th parallel of north latitude, if there be one, as alleged by some writers.
12. The location of a calm at the north pole, if there be one, as Professor Ferrel believes.

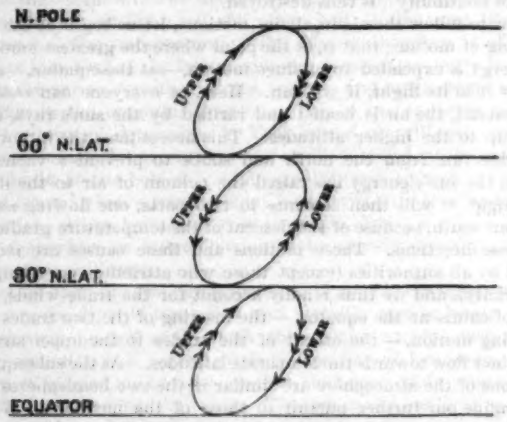
Now it will require but slight reasoning to prove that a body of air starting from the equator can never reach the pole. Take, for example, the amount of air included between any two meridians of longitude; at the equator, the distance between these lines is about or near 70 miles. This body of air, as a return trade or upper current, starts moving toward the north. Concede for a moment that the earth does not rotate upon its axis. This air in its northern flight cannot veer to the right or left beyond these lines, for there is always another body of air there on either side to prevent it. As the meridians continually approach each other, and at the pole unite, it will be readily seen that if the air should ever reach the pole under these circumstances, it must be squeezed into space where there is no space. The length of the equatorial line is about 25,000 miles. The air starts all along this line to move northward simultaneously; can it keep on converging, until the entire 25,000 miles of air shall be forced into a space represented by a dot? It is physically impossible. A million cubic miles of air cannot be compressed into a pint cup. Conversely, a cubic foot of air can cover the north pole; if this amount should endeavor to reach the equator, it must, when it arrives there, expand to the extent of 25,000 miles. The theory of Professor Ferrel—the interchange between the poles and the equator—requires that the air at the earth's surface in all northern latitudes should move southerly with a western component of motion, similar to the trades; but the fact is, as everyone knows, and as he himself admits, that in the north temperate zone the surface winds move northward with an eastern component of motion; that is, from the south-west. This motion is shown by the arrows in the figure on page 155 of his book, referred to in the above quotation. Furthermore, the arrows in the same figure indicate that he would have the motion of all surface winds except the trades at the tropics, and all the upper strata, without exception, towards the east, the surface winds moving north-east, and the upper strata moving nearly east, but a little north of east; and he endeavors to prove this to be true, by elaborate reasoning and mathematical formulæ, extending over many pages. But this easterly motion of nearly the entire atmosphere is directly opposed by another principle which he recognizes on page 117, where he says: "This principle was recognized by Hadley in his theory of the trade-winds, for he states that *all motions* in any direction must have their counter-motions, else the effect upon the earth's surface would be to change the earth's rotation upon its axis." If it be objected that this reasoning is intended only to apply to surface winds, it may be stated that Professor Ferrel, on pages 93 and 94, seems to recognize the broad principle that *all* motions must have their counter-motions, which he terms the "condition of continuity." It is difficult to conceive how it is possible that all the upper strata, from pole to pole, should move in one direction, and that direction east, and so rapidly as to get ahead of the motion of the earth in its rotation upon its axis. Where are the counter-currents to all these easterly winds? Nowhere, except the surface trade-winds within the tropics. What force, what energy is it that causes all the winds to flow in one direction, and at so rapid a rate? What is their source of supply? and what is their limit of range or destination? Since their motion, as alleged, is not directly east, but a little north and south, in the respective hemispheres, of an east and west parallel, will they not ultimately fly off from the earth at the poles? The western component of motion of the trades, he says, is balanced by the corresponding eastern component of the winds in the more northern latitudes, and there is kept up and maintained his "condition of continuity," so far. But as there are no counter-currents alleged to correspond to the easterly motion of all the upper strata, it seems that his "condition of continuity" is thus destroyed.

Now to follow the air in all its motions, let us begin at the beginning of motion; that is, at the point where the greatest amount of energy is expended to produce motion,—at the equator,—and follow it in its flight, if we can. Here, as everyone can readily understand, the air is heated and rarified by the sun's rays, and rises up to the higher altitudes. This necessitates the inflowing of colder air from the north and south to prevent a vacuum. When the sun's energy has raised the column of air to the limit in height, it will then separate in two parts, one flowing north and one south, because of the descent of the temperature gradients in those directions. These motions and these causes are recognized by all authorities (except those who attribute everything to electricity), and we thus readily account for the trade-winds, the belt of calms at the equator,—the meeting of the two trades destroying motion,—the ascent of the trades to the upper strata, and their flow towards the temperate latitudes. As the subsequent motions of the atmosphere are similar in the two hemispheres, let us confine our further pursuit to those of the northern. When the upper stratum begins moving northward, it has acquired the eastern motion of the earth near the equator, and soon coming over portions of the earth with slower motion, it will get ahead of the earth and veer to the north-east,—making the return trades,—the cause being the reverse of that which produced the south-west motion of the trade-winds. As the parallels of latitude grow shorter and the meridians of longitude approach each other as we go north from the equator, it is evident that this body of air will soon become confined into narrower limits than it had at the equator; and it will sometime and somewhere happen that it will become so heaped up and crowded that its northern motion will be retarded, and finally cease entirely. When this happens, it will find the line of least resistance down towards the surface, where it will go to keep up the supply of the trades, and will then return to the equator again. So it may be inferred that this body of air will thus continue to make its eternal round in this grand cycle, unless changed from its course by local causes, topography, unequal distribution of temperature, etc. Its most northern limit is believed to be about the parallel of 30°, and its heaping up and downpour will cause both a calm and a high barometric pressure at this parallel.

Turning our attention next to the atmosphere in the north temperate latitudes, we discover that the air to the north of the above-named calm belt has a north-east motion at the surface of the earth. This eastern component of motion is doubtless produced by the same causes that operated to give a north-east motion to the upper strata of the tropics. When this body of air has proceeded for some distance to the northward, it will also be confined by the shortening of the parallels of latitude and the nearer approach of the meridians of longitude; and, thus meeting with resistance in front, it seeks the line of least resistance in the upper

air, whither we follow it in an ascending current. It being perceived that its departure from the 30th parallel tended to produce a vacuum on that line, this current of air flows back again as upper strata in a south-west direction, obeying the same law which gives a western component of motion to the trades; when it reaches the parallel of 30° and then meets the other body from the equator, its further progress in that direction ceases, and it pours down to the surface and begins its circuit again. The northern limit of this motion is believed to be about the 60th parallel. So here we have another body of air, similar to that within the tropics, moving in a continuous circuit, but in opposite directions. For similar reasons, the atmosphere between latitude 60° and the pole will also move in a circuit between those points, only the direction of motion will be the reverse of that in the temperate zone. The coldest air in this northern zone being presumably at the pole, and therefore heaviest, will sink down to the surface and move southward with a western component, obeying the same laws heretofore given. When it reaches latitude 60°, it will meet the current from the south, rise to the upper strata, and flow back to the pole.

These several motions and the entire circulation in the northern hemisphere may be better understood by reference to the following figure: —



The arrows at the right hand show the direction of the surface flow, and those at the left indicate the direction of the upper strata, in the several zones.

It is to be understood, of course, that the foregoing theory is based upon the assumption of an earth with a homogeneous surface in both hemispheres; and that any variations from these results are due to differences of temperature, topography, etc. The existence of these three zones of air currents, with motions as here proposed, seems to furnish a full explanation of most of the facts known and observed up to date. That there is a belt of calms at the 30th parallel, and also a high barometer, seems to be so well established that no one can be found with the temerity to deny it. If there be an interchange of air between the poles and the equator, by a surface flow southward and upper strata flowing north, as proposed by Professor Ferrel and others, it seems impossible to explain the existence of this calm belt and high barometric pressure at parallel 30, or at any other place between the equator and the pole. The air flowing horizontally across any particular locality cannot produce a calm or a high pressure at that locality, whatever the velocity may be. A calm is produced by the meeting or parting of winds; a high pressure is produced by a down-pour, and a low pressure by an up-pour of the air.

So, also, the prevailing winds in the north temperate latitudes, from the south-west to the north-east, are so well established, that it is deemed no evidence is required here to prove their existence. Their direction and motion cannot be explained on Professor Ferrel's theory of a southward tendency of the air in that zone at the surface. A calm at the pole might be reasonably deducible from his theory; but one at the 60th parallel is impossible.

T. A. BERRMAN.

Mount Pleasant, Ia, May, 1892.

Four-Fold Space.

IN the May 13 number of *Science*, I find a very interesting discussion of "The Possibility of a Realization of Four Fold Space," being a digest of a paper by Dr. T Proctor Hall. As I have not had the pleasure of reading Dr. Hall's paper, and as I have not read any fourth-dimensional literature for quite a while, what I am about to say may be old. If not, and you find it worthy of publication, you may use it.

All modern thinkers about the Kantian philosophy of the fourth dimension of space, have, I suppose, dipped more or less into Professor Zöllner's Transcendental Physics. It looks as if Dr. Hall had done so, as his discussion of the knotted-string question and the "plane being" as distinguished from an ordinary three-dimensional mortal, is quite similar to certain illustrations used by Professor Zöllner.

I think Dr. Hall's idea of trying to get a clear concept of fourth-dimensional space, by initial projections from three-dimensional space, and then modifying those projections as best we can, is very ingenious, and may become a very useful factor in the study of the possibilities of four-dimensional space and four-dimensional beings; and I think he is entitled to great credit for his clear and effective start made in that direction.

I have only one criticism to make about it, which is that such a process would be exceedingly slow, as slow as the building up of the science of mathematics, or chemistry, or any other science which had to start with wholly unknown premises. I do not think that the study of four-fold space absolutely requires treatment of this elementary character. This opinion is based upon the following thoughts and inferences, which I have from time to time drawn with regard to this fourth dimension, and made use of in private conversation with regard thereto.

The so-called universe of matter, as has been repeatedly said, is known to us only because there is an unknown x (whether force or substance we cannot tell), which successfully resists our attempts to penetrate it, whether the attempt be made by the sight, the touch, or such power of projectile force as we think we have succeeded in bringing under our control. Outside of this resistance there is absolutely nothing but inference, an inference which some philosophers regard as amounting to conviction, and others, not.

When we say a block of granite is impervious or impenetrable, we simply announce an inference mentally drawn from impressions received by our various organs; and the point which I am now raising is simply this: that the same impressions might be received, and hence the same inference drawn, under a totally different state of affairs, provided we assume — and we have no reason for not assuming — that our standards, such as a foot of twelve inches, an inch of three barley-corns, etc., are simply relative, and compared with the infinite universe mean absolutely nothing, in other words, are not standards at all. Not to make this too long, but to illustrate hurriedly where I have thought, for some years, a starting point for the practical demonstration of four-dimensional space may be found, let me use an illustration.

Let us call our granite block a ten-foot cube. Standing in front of it we can only see one side; at a certain angle we can see two sides. From an elevated point we can see two sides and the top; but we can never see, except by the aid of reflectors, more than three of the six sides at once. We can easily walk around and under it, and see the other sides. In other words, and this is the key of the whole situation, we can see the whole of the cube successively but never simultaneously; and this applies to the inside as well as the outside. If this granite block were magnified so that each dimension was a thousand times what we have assumed it to be, it might be a very porous and loosely-jointed structure; yet if our eye were placed with increased faculties at a proper distance, the phenomenon presented to that eye would be exactly that which now shines forth in the ten-foot block of granite, and our inference as to its size and structure would be identical with our first assumption.

As we have no difficulty in believing that, owing to the revolution of the earth combined with its motion around the sun, we have been carried many miles through space in the fraction of a second which elapses, as we think, between dropping a coin and

picking it up again, why should we regard it as an incredibly extravagant assumption that a correspondingly large space is unconsciously travelled over when we walk from one side to the other of our granite block? As the glimpse which we get of some of the fixed stars is merely a ray of light which has taken many hundred years to reach us, why should it be an altogether unreasonable assumption that the light-ray from our granite block may take a good deal longer to reach us than we are aware of? As we know, from experiments with birds, that there are sounds too high-pitched for our ear to detect, is it not in every way natural to expect that there are dimensions which the eye cannot detect?

To sum up: As our inferences with regard to the material world are rather the result of the limitations of our faculties than limitations of so-called matter itself, are we not likely to get ahead faster in the effort to broaden our concepts, and with them our ability to form concepts, by modifying our inferences than by trying to project our inferences into an unknown dimension?

W. P. PREBLE.

New York, May 22.

H. Carvill Lewis's Work on the Glacial Phenomena.

THE following communication from the wife of the late Professor Lewis seems to me worthy of publication, both out of respect to the writer and for the considerable amount of valuable information which it contains upon a subject that is now uppermost in the minds of a considerable portion of the geological world. I have no doubt that a large circle of your readers will read it with great interest.

G. F. WRIGHT.

Oberlin, Ohio, May 23.

PROFESSOR G. FREDERICK WRIGHT, LL.D.

Dear Sir:— YOUR valuable reprint from the *Journal of Science* for January, 1892, on "The Theory of an Inter-Glacial Submergence in England" was duly received this morning, and after a careful perusal of its contents I hasten to thank you for your courtesy in sending it.

The many questions relative to the causes and extent of the great glacial epoch have, with its accompanying phenomena, occupied a large share of my thoughts during the past twelve years, first, because of its surpassing interest and close connection with the solution of some of the most important physical and astronomical problems of the day; and, second, because I had the pleasure of sharing all my husband's ideas and plans and much of his field-work, from the day when he first made your acquaintance at the Boston meeting of the American Association, in August, 1880, to July 17, 1888, when, knowing the precarious nature of the malady which had attacked him, he gave all his unfinished manuscripts into my care, with the request, that, as I knew his inmost wishes in regard to them, I would see that they were all completed and published as they ought to be. The MS. for my husband's "Observations on the Glacial Phenomena of Great Britain"—so ably edited by the Rev. Dr. Crosskey of Birmingham, and covering 1,100 pages of foolscap, has been in Washington since July last awaiting publication. Had it been printed before the paper which you have so kindly sent me was written, I think you would have obtained a slightly different impression of my husband's later views from that expressed in the closing paragraphs.

As the importance of clear definition in scientific work of all kinds can hardly be overestimated, and as my husband's one wish was to learn the truth irrespective of theories, which he regarded merely as tentative hypotheses, to be thrown aside when they no longer served the purpose for which they had been constructed, I think that the term, "Correction of some of Professor Lewis's earlier working hypotheses," would give a clearer impression of the real state of the case than the phrase, "Correction of Professor Lewis's personal equation," which to the world in general implies a constant and known element of error in all that an observer sees or does, and which must be strictly accounted for in the sum-total of his work.

As no one could be more anxious than I am (except my husband himself) that all errors of whatever sort shall be promptly eliminated from his life work, and as I have only too good reason for knowing the endless and varied misconceptions with regard to his views, which have naturally arisen from the fragmentary reports of his European observations that have hitherto been published, I think that it may aid not only yourself but the scientific world generally if I send you a short synopsis of his later opinions. These are briefly as follows:—

With regard to the terminal moraine in Pennsylvania, over the last third of which he enjoyed the great pleasure and advantage of your companionship, his opinion remained unchanged, that a well-defined moraine had throughout the State defined the line of the solid ice-front.

The varying line of bowlders, scattered about as plums over a pudding, found considerably south of the moraine at different points in the western portion of the State, and which you both decided to name "The Fringe," he at first suggested (see Report Z) had been caused by a projection of the upper layers of ice—which move more rapidly than those beneath them—over the lower layers, which, as the ice rose hundreds of feet higher than the moraine at its base, would naturally and in accordance with its proper motion project the bowlders on the surface lying beyond the moraine line.

This view, however, was merely a tentative one, as he himself confessed (see Report Z), and he abandoned it in 1886, as his investigation of the English glacial deposits drew toward a close.

From many similar instances of "fringe" observed in Great Britain, and also in Switzerland and northern Italy, he was thoroughly convinced that the phenomena in each case that he himself examined had been caused by the damming back of streams flowing toward the ice-front and forming bodies of water of varying size and depth, which he called "extra-moraine lakes."

Full details and diagrams relating to his studies of these will be found in the forthcoming volume, and also his application of them to the phenomena observed in western Pennsylvania, where like features occur. The deposit of bowlders over the beds and along the edges of these extra-moraine lakes he held to be largely due to the drifting and melting of detached bergs, or cakes of ice, from the foot of the glacier, in which the *débris* had been frozen, or on whose surfaces the bowlders had been perched.

I do not remember my husband at any time thinking that "the fringe was the remnant of an earlier and distinct glacial period," though in the Old World he found in many places very clear evidence of there having been an advance or retreat, and a second advance of the isolated or coalescing streams, which together gave rise to the phenomena of the great glacial period.

I do, however, recall his frequent statement that never in any of his personal observations in America, Ireland, Great Britain, Switzerland, or Italy had he found a single instance of a glacier, ancient or modern, which had not at the time of its greatest extension been marked by a moraine at the foot of the solid ice, though these moraines often showed the greatest variety of form, from a low, flat deposit of gravel, sand, or till, from a few feet to a mile in width, and from a tiny ridge over which a man could easily step to the gigantic drift hills of northern Italy.

Exceptions to these observations occurred in cases where the ice moved from the land into the sea, as on the south side of the Killarney ice-centre, on the west side of the Clare Mountains, and in other instances, of which he himself has left a full description. The moraine in some portions of western England was much disturbed by the alternate elevation, depression, and re-elevation of that section of the country during the period of maximum glaciation, which caused a mingling and interbedding of morainic and marine deposits. Special stress should here be laid upon my husband's qualifying expression, "in my own experience," for he never at any time denied that a glacier ever had existed, did now exist, or could exist in the future without being bounded by a terminal moraine; he simply said, "I, personally, have been unable to find one."

With regard to your own admirable work in the State of Ohio, and beyond it toward the Mississippi valley, where the ice-front had not been marked by any definable moraine,—owing to its having gradually lost momentum and become very much attenuated in passing over a long, wide, and gently sloping plain till practi-

cally nothing remained of it,—my husband was fully prepared to accept the conclusions to which you had been led for that particular section of the country, as you will see from the notes appended to his English work.

In Ohio a set of conditions occur wholly unlike anything which my husband himself had seen in his glacial work, and making the allowance for a different glacial behavior, such as these conditions demanded, he felt that your own opinion with regard to them was the most logical he could reach at that time. In England, Wales, and Ireland a terminal moraine everywhere bounded the absolute ends of the separate or coalescing tongues of ice, except, as I have stated, where the ice had passed off to sea, or the moraine deposits had been disturbed by contemporary or subsequent water action, of which, in either case, there was always more or less distinct evidence. The moraine lines mentioned in your paper are all given in full in my husband's sketch of "The Terminal Moraines of the Great Glaciers of England," published for the Meeting of the British Association in Manchester in September, 1887; and his later opinions as to the origin of the "fringe" will be found in a similar article on "Some Great Extra-Morainic Lakes in England and North America at the Time of Maximum Glaciation."

My husband distinctly held that the maximum submergence in the West of England had attained a depth of from 450 to 500 feet, but had not reached that of 1,000 feet or more, as claimed by some of the leading British geologists.

Another point to be emphasized is that in my husband's mind a terminal moraine showed the halting-place of the solid ice only at the time of its greatest extension, and did *not* define or limit the irregular drift-covered areas in many instances found lying beyond it, which were due to the action of drainage-streams, icebergs or the deposits in temporary lakes.

As I have elsewhere stated, the first and only instance my husband ever saw which led him to believe in the existence of a large ice-stream (whether local or otherwise remains to be determined), between which and the glacial epoch as vast an interval of time had elapsed as that which separates the glacial period from the present day, was found in the deposits on Frankley Hill, near Birmingham. It was his intention, had he remained in this world, to make a thorough re-examination of all England, lest similar deposits had elsewhere escaped his notice; but he never at any time associated the Frankley Hill till and gravel with the "fringe" of the glacial period, from which it was wholly distinct.

Permit me to say in closing that the unlimited courtesy and generosity shown me by Dr. Crosskey and many others among the English geologists—some of whom are entirely opposed to my husband's conclusions—are beyond all praise and any acknowledgment which it is in my power to give. If, when I have in future to turn to my own countrymen for aid in finishing my husband's MSS. relating to the geology of the New World, I experience even a fraction of the kindness which has surrounded me in England, I shall have nothing left to desire.

CALENDAR OF SOCIETIES.

Philosophical Society, Washington.

May 21.—H. A. Hazen, Scientific Ballooning; Alexander S. Christie, The Method Employed to Find the Latitude-Variation Tide.

Publications Received at Editor's Office.

Abbott, Lyman. The Evolution of Christianity. Boston, Houghton, Mifflin & Co. 12°. 266 p. $1.25.

Bailey, L. H. Cross-Breeding and Hybridiging. New York, Rural Pub. Co. 12°, paper. 44 p. 40 cts.

Holbrook, M. L. The Hygienic Treatment of Consumption. New York, M. L. Holbrook & Co. 12°. 219 p.

Lydekker, R. Phases of Animal Life Past and Present. New York, Longmans, Green & Co. 12°. 248 p. $1.50.

New Jersey. Annual Report of the State Geologist for 1891. Trenton, J. L. Murphy Pub. Co., printers. 8°, paper. 278 p.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 874 Broadway, New York.]

Taxidermist going out of business has quantity of finely-mounted specimens of North American birds, mammals and reptiles and skins of birds for sale, including a full local collection of bird skins, showing some great variations of species; also quantity of skulls with horns of deer and mountain sheep, and mounted heads of same. Will give good exchange for Hawk Eye camera with outfit. Apply quickly to J. R. Thurston, 265 Yonge St., Toronto, Canada.

For exchange.—A fine thirteen-keyed flute in leather covered case, for a photograph camera suitable for making lantern slides. Flute cost $27, and is nearly new. U. O. COX, Mankato, Minn.

To exchange: Experiment Station bulletins and reports for bulletins and reports not in my file. I will send list of what I have for exchange. P. H. ROLFS, Lake City, Florida.

Finished specimens of all colors of Vermont marble for fine fossils or crystals. Will be given only for valuable specimens because of the cost of polishing. GEO. W. PERRY, State Geologist, Rutland, Vt.

For exchange.—Three copies of "American State Papers Bearing on Sunday Legislation," 1891, $2.50, new and unused, for "The Sabbath," by Harmon Kingsbury, 1840; "The Sabbath," by A. A. Phelps, 1842; "History of the Institution of the Sabbath Day, Its Uses and Abuses," by W. L. Fisher, 1859; "Humorous Phases of the Law," by Irving Browne; or other works amounting to value of books exchanged, on the question of governmental legislation in reference to religion, personal liberty, etc. If preferred, I will sell "American State Papers," and buy other books on the subject. WILLIAM ADDISON BLAKELY, Chicago, Ill.

For Sale or Exchange for books a complete private chemical laboratory outfit. Includes large Becker balance (200g. to 1-10mg.), platinum dishes and crucibles, agate motors, glass-blowing apparatus, etc. For sale in part or whole. Also complete file of *Silliman's Journal*, 1862-1885 (62-71 bound); Smithsonian Reports, 1854-1883; U. S. Coast Survey, 1854-1880. Full particulars to enquirers. F. GARDINER, JR., Pomfret, Conn.

Wanted, in exchange for the following works, any standard works on Surgery and on Diseases of Children: Wilson's "American Ornithology," 3 vols.; Coues' "Birds of the Northwest" and "Birds of the Colorado Valley," 2 vols.; Minot's "Land and Game Birds of New England;" Samuels' "Our Northern and Eastern Birds;" all the Reports on the Birds of the Pacific R. R. Survey, bound in 2 vols., morocco; and a complete set of the Reports of the Arkansas Geological Survey. Please give editions and dates in corresponding. R. ELLSWORTH CALL, High School, Des Moines, Iowa.

To exchange Wright's "Ice Age in North America" and Le Conte's "Elements of Geology" (Copyright 1882) for "Darwinism," by A. R. Wallace, "Origin of Species," by Darwin, "Descent of Man," by Darwin, "Man's Place in Nature," Huxley, "Mental Evolution in Animals," by Romanes, "Pre-Adamites," by Winchell. No books wanted except latest editions, and books in good condition. C. S. Brown, Jr., Vanderbilt University, Nashville, Tenn.

Wants.

Any person seeking a position for which he is qualified by his scientific attainments, or any person seeking some one to fill a position of this character, be it that of a teacher of science, chemist, draughtsman, or what not, may have the 'Want' inserted under this head FREE OF COST, *if he satisfies the publisher of the suitable character of his application. Any person seeking information on any scientific question, the address of any scientific man, or who can in any way use this column for a purpose consonant with the nature of the paper, is cordially invited to do so.*

WANTED.—By a young man, a Swarthmore College junior, a position as principal of a public high school in one of the Gulf States, or as instructor in botany, physiology, and geology in an academy or normal school. Address B., care of Librarian, Swarthmore College, Penn.

WANTED.—A teacher of Geology who is familiar with the fossils of the Hamilton Group, as instructor of Geology during July next at the Natural Science Camp on Canandaigua lake. Apply to ALBERT L. AREY, Director, 229 Averill Ave., Rochester, N. Y.

WANTED.—To act as correspondent for one or two daily or weekly papers. Have worked on paper for about two years. Would like a position on editorial staff of humorous paper. Address GEO. C. MASON, 14 Elm St., Hartford, Conn.

TRANSLATOR wanted to read German architectural works at sight (no writing). One familiar with technical terms desired. Address "A.," Box 149, New York Post Office.

WANTED.—A position in a manufacturing establishment by a manufacturing Chemist of inventive ability. Address M. W. B., care of *Science*, 874 Broadway, N. Y.

WANTED.—Books on Anatomy and Hypnotism. Will pay cash or give similar books in exchange. Also want medical battery and photo outfit. DR. ANDERSON, 182 State street, Chicago, Ill.

WANTED.—A college graduate with some normal training, to teach the sciences, at $1,800 per year, in a Southern college. A Baptist or a Methodist preferred. Must also be a first-class Latin scholar. A. H. Beals, Box K, Milledgeville, Ga.

A PROFESSORSHIP in Chemistry is wanted by one who has had five years' experience in that capacity. Would prefer to give instruction by lectures and experiments rather than by text-book methods. Would like a position in a college or university where there is a good student's laboratory. Special points of strength claimed are: (1) Thorough control of a class and good order during lectures and recitations. (2) Accuracy in experimenting with chemicals and skill in the manipulation of chemical apparatus. The permission of several distinguished educators has been given to refer to them if required. Would not care to accept a position paying less than $1,500. Address B. E., care of *Science*, 874 Broadway, New York.

ADDRESS WANTED.—Will some one please send the address of the Secretary of the American Philological Society. Also that of Herbert Spencer. "ADDISON," Room 84, 164 Madison St., Chicago, Ill.

As the publication of this letter may serve to elucidate my husband's views and to explain what his exact position was with regard to the leading questions of the day in glacial geology, pending the publication of his own work, I shall be greatly indebted if you will insert it at such a place in your detailed defense of his views as your own greater wisdom shall direct.

Again thanking you for your interesting and valuable paper, believe me to be, with regard,

Faithfully yours,

JULIA F. LEWIS.

Hotel Lang, Heidelberg.

AMONG THE PUBLISHERS.

THE fourth number of the "Columbia College Studies in Political Science," completing the first volume of that series of monographs, is entitled "The Financial History of Massachusetts from the Organization of the Massachusetts Bay Company to the American Revolution," by Charles H. J. Douglas, Ph.D., Seligman Fellow in Political Science in Columbia College. Doctor Douglas, before he returned East some four or five years ago to take charge of the work in history and English literature in the Brooklyn Boys' High School, was proprietor and managing editor of the *University*, a weekly literary and critical journal of Chicago, since merged in *Unity*, the well-known liberal religious weekly of that city. The *University*, during the two or three years of its separate existence, gained a high position as an independent medium of scholarly discussion. Besides Doctor Douglas, then an instructor in the University of Wisconsin, its editorial staff included the late Professor Alexander Winchell, of the University of Michigan; Professor William H. Payne, now chancellor of the University of Nashville; Professor Charles K. Adams, lately president of Cornell University, and Professor George W. Knight, now of the Ohio State University, all of whom contributed to each number. Complete volumes of the *University* are now excessively rare.

—The next annual meeting of the Royal Society of Canada will be held at Ottawa on May 31 and following days, and will be opened with the usual inaugural address by the President, the Reverend Abbé Laflamme. Amongst the papers to be presented the following are of scientific interest: In the section of English literature a vocabulary of the language of the Beothicks, or Red Indians of Newfoundland, by the Rev. Dr. Patterson, and a grammar and dictionary of the language of the Haida Indians of the Queen Charlotte Islands, by Rev. Chas. Hamilton of British Columbia; in the Physical section, "The Fundamental Hypothesis of Abstract Dynamics," by Professor J. G. MacGregor; "Long Columns," by Professor Bovey; and "On a New Form of Application Goniometer," by Professor Chapman; and in the Geological and Biological section, "The Fossils of the Hudson River Formation in Manitoba," by J. F. Whiteaves, and "On the Correlation of Early Cretaceous Floras in Canada and the United States, and on Some New Plants of this Period," by Principal Sir William Dawson.

QUERY.

Can any reader of *Science* cite a case of lightning stroke in which the dissipation of a small conductor (one-sixteenth of an inch in diameter, say,) has failed to protect between two horizontal planes passing through its upper and lower ends respectively? Plenty of cases have been found which show that when the conductor is dissipated the building is not injured to the extent explained (for many of these see volumes of Philosophical Transactions at the time when lightning was attracting the attention of the Royal Society), but not an exception is yet known, although this query has been published far and wide among electricians.

First inserted June 19. No response to date.

TO THE READERS OF SCIENCE.

PUBLISHER'S ANNOUNCEMENT.

OUR PLANS.

Within the past six months the use of *Science* by scientific men and women as a medium for prompt publication and weekly discussion, has increased very materially, so that the pages are now well filled each week with original matter. As the number of those promising contributions is increasing at the rate of three or four each day, it cannot be long before *Science* at its present size will be too small for the amount of matter offered. We have under consideration therefore an enlargement of the paper by one-half, but must first learn the temper of our constituency as to an advance in price to $5.00, which was the subscription price from the start for four years, up to June 30, 1887. Further, to carry out the proposed enlargement, we shall need five hundred additional subscribers. If you are not already a subscriber, are you willing to aid in making *Science* more worthy of American scientific work by becoming one?

It goes without saying, that the demand for scientific literature is limited, when compared with that for literature which is more to the public taste, so that the receipts of most of the Scientific Journals, in this country, do not pay quite for their printing and paper, to say nothing of the other items of expense. We say this merely to emphasize the fact, that generous and prompt support must be accorded this move if it is to succeed.

Titles of Some Articles Published in *Science* since Jan. 1, 1892.

Aboriginal North American Tea.
Actinism.
Amenhotep, King, the tomb of.
Anthropology, Current Notes on.
Arsenical Poisoning from Domestic Fabrics.
Anatomy, The Teaching of, to Advanced Medical Students.
Astronomical Notes.
Botanical Laboratory, A.
Brain, A Few Characteristics of the Avian.
Celts, The Question of the.
Collection of Objects Used in Worship.
Deaf, Higher Education of the.
Diphtheria, Tox-Albumin.
Etymology of two Iroquoian Compound Stems.
Eye-Habits.
Family Traits, Persistency of.
Fishes, The Distribution of.
Fossils, Notice of New Gigantic.
Grasses, Homoptera Injurious to.
"Healing, Divine."
Hemipterous Mouth, Structure of the.
Hypnotism among the Lower Animals.
Hypnotism, Traumatic.
Indian occupation of New York.
Influenza, Latest Details Concerning the Germs of.
Infant's Movements.
Inventors and Manufacturers, the American Association of.
Iowa Academy of Sciences.
Jargon, The Chinook.
Klamath Nation, Linguistics.
Lightning, The New Method of Protecting Buildings from.
Lissajou's Curves, Simple Apparatus for the Production of.
Maize Plant, Observations on the Growth and Chemical Composition of.
Mineral Discoveries, Some Recent, in the State of Washington.
Museums, The Support of.
Patent Office Building, The.
Pocket Gopher, Attempted Extermination of.
Psychological Laboratory in the University of Toronto.
Psychological Training, The Need of.
Rain-Making.
Rivers, Evolution of the Loup, in Nebraska.
Scientific Alliance, The.
Star, The New, in Auriga.
Storage of Storm-Waters on the Great Plains.
Teaching of Science.
Tiger, A New Sabre-Toothed, from Kansas.
Timber Trees of West Virginia.
Tracheæ of Insects, Structure of.
Vein-Formation, Valuable Experiments in.
Will, a Recent Analysis of.
Wind-Storms and Trees.
Wines, The Sophisticated French.
Zoology in the Public Schools of Washington, D. C.

Some of the Contributors to *Science* Since Jan. 1, 1892.

Aaron, Eugene M., Philadelphia, Pa.
Allen, Harrison, Philadelphia, Pa.
Baldwin, J. Mark, University of Toronto, Canada.
Barnes, Charles Reid, Madison, Wis.
Baur, G., Clark University, Worcester, Mass.
Beal, W. J., Agricultural College, Mich.
Beals, A. H., Milledgeville, Ga.
Beauchamp, W. M., Baldwinsville, N.Y.
Boas, Franz, Clark University, Worcester, Mass.
Bostwich, Arthur E., Montclair, N.J.
Bradley, Milton, Springfield, Mass.
Brinton, D. G., Philadelphia, Pa.
Chandler, H., Buffalo, N.Y.
Comstock, Theo. B., Tucson, Arizona.
Cragin, F. W., Colorado Springs, Col.
Davis, W. M., Harvard College, Cambridge, Mass.
Dimmock, George, Canobie Lake, N.H.
Farrington, E. H., Agricultural Station, Champaign, Ill.
Ferree, Barr, New York City.
Flexner, Simon, Johns Hopkins University, Baltimore, Md.
Foshay, P. Max, Rochester, N.Y.
Gallaudet, E. M., Kendall Green, Washington, D.C.
Garman, S., Museum of Comp. Zool., Cambridge, Mass.
Golden, Katherine E., Agricultural College, Lafayette, Ind.
Hale, Edwin M., Chicago, Ill.
Hale, George S., Boston, Mass.
Hale, Horatio, Clinton, Ontario, Canada.
Hall, T. Proctor, Clark University, Worcester, Mass.
Halsted, Byron D., Rutgers College, New Brunswick, N.J.
Haworth, Erasmus, Oskaloosa, Iowa.
Hay, O. P., Irvington, Ind.
Haynes, Henry W., Boston Mass.
Hazen, H. A., Weather Bureau, Washington, D.C.
Hewitt, J. N. B., Bureau of Ethnology, Washington, D.C.
Hicks, L. E., Lincoln, Neb.
Hill, E. J., Chicago, Ill.
Hill, Geo. A., Naval Observatory, Washington, D.C.
Hitchcock, Romyn, Washington, D.C.
Hotchkiss, Jed., Staunton, Va.
Howe, Jas. Lewis, Louisville, Ky.
Hubbard, Gardiner G., Washington, D.C.
James, Joseph F., Agricultural Dept., Washington, D.C.
Johnson, Roger B., Miami University, Oxford, O.
Kellerman, Mrs. W. A., Columbus, O.
Kellicott, D. S., State University, Columbus, O.
Macloskie, G., Princeton, N.J.
McCarthy, Gerald, Agricultural Station, Raleigh, N.C.
MacDonald, Arthur, Washington, D.C.
Marshall, D. T., Metuchen, N.J.
Mason, O. T., Smithsonian Institution, Washington, D.C.
Nichols, C. F., Boston, Mass.
Nuttall, George H. F., Johns Hopkins University, Baltimore, Md.
Oliver, J. E., Cornell University, Ithaca, N.Y.
Osborn, Henry F., Columbia College, New York City.
Osborn, Herbert, Agricultural College, Ames, Iowa.
Pammel, L. H., Agricultural Station, Ames, Iowa.
Pillsbury, J. H., Smith College, Northampton, Mass.
Ruffner, W. H., Lexington, Va.
Schufeldt, R. W., Washington, D.C.
Scripture, E. W., Clark University, Worcester, Mass.
Slade, D. D., Museum Comp. Zool., Cambridge, Mass.
Smith, John B., Rutgers College, New Brunswick, N.J.
Stevenson, S. Y., Philadelphia, Pa.
Stone, G. H., Colorado Springs, Col.
Thurston, R. H., Cornell University, Ithaca, N.Y.
Todd, J. E., Tabor, Iowa.
True, Frederick W., National Museum, Washington, D.C.
Turner, C. H., University of Cincinnati, Cincinnati, O.
Ward, P. DeC., Harvard University, Cambridge, Mass.
Ward, Stanley M., Scranton, Pa.
Warder, Robert B., Howard University, Washington, D.C.
Welch, Wm. H., Johns Hopkins University, Baltimore, Md.
West, Gerald M., Clark University, Worcester, Mass.
Williams, Edward H., Lehigh University, Bethlehem, Pa.